

Life History Variation in Steelhead Trout and the Implications for Water Mangement

Marc Steven Mangel

Final Selection Panel Review

Proposal Title

#0140: Life History Variation in Steelhead Trout and the Implications for Water Mangement

Funding:

Fund in part

Amount: \$1,026,095

The final Selection Panel concurred with its initial findings on this proposal. Due to the reduction in funds available for the Science Program's 2004 PSP, the Selection Panel recommended funding for this proposal be reduced to a recommended amount of \$1,026,095. Should the California Bay-Delta Authority accept the Selection Panel's recommendation and approve the funding of this proposal, the applicant will be allowed to negotiate which tasks and associated costs will be reduced as part of the contracting process.

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0140: Life History Variation in Steelhead Trout and the Implications for Water Management

Funding:

Fund

Amount: \$1,136,095

Initial Selection Panel (Primary) Review

Topic Areas

- Life Cycle Models And Population Biology Of Key Species
- Environmental Influences On Key Species And Ecosystems
- Relative Stresses On Key Fish Species
- Direct And Indirect Effects Of Diversions On At-risk Species
- Water Management Models For Prediction, Optimization, And Strategic Assessments
- Salmonid-related Projects

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

The proposed project would develop a life-cycle model of steelhead, a listed species in California's Central Valley. The project would address environmental influences and stresses on life history variation in steelhead. The project would improve the understanding of the effects of efforts to manage instream flows and temperature, and would provide an important tool that could be used to improve management of river systems for the benefit of steelhead.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the

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Initial Selection Panel Review

proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

Technical reviewers questioned the level of support for the primary investigator and costs associated with equipment for invertebrate sampling. Also, overhead rates appear to vary dependent on the primary entity conducting the tasks (49% for UCSC and 26.4% for NMFS). The Science Program should consider negotiating reduced salary support for the primary investigator and reduced overhead rates.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

The proposed project would provide valuable information on steelhead life history variation that would likely have important management implications. Overall, the study was well designed, essentially based on successful and informative work on Atlantic salmon. Although the technical reviews expressed concerns over the lack of detail for some aspects of the laboratory and field experiments, they also expressed confidence that the applicants were capable of conducting the work based on their prior performance.

Selection Panel (Discussion) Review

fund this amount: \$1,136,095

note:

fund

The Panel appreciated the goals and objectives of this research. The Panel felt that it is critical to understand the behavioral and physiological responses of steelhead that produce life-history variation in this species. The research team is well-qualified to conduct this research; the PI has successfully completed similar modeling efforts on salmonids in Europe. This research has a high likelihood of success and, if successful the products will have important steelhead and

Initial Selection Panel Review

water management implications.

Technical Synthesis Panel Review

Proposal Title

#0140: Life History Variation in Steelhead Trout and the Implications for Water Mangement

Final Panel Rating
above average

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The goals of this study are to determine, using laboratory, field and modeling studiues; (1) how environmental factors determine steelhead life history transitions; (2) how steelhead life history patterns vary across populations and watersheds, (3)how flow modification affects growth opportunity and life history decisions; and 4) how life history transitions affect steelhead population dynamics. The justification for this project is high, as steelhead populations have continued to decline since dams were constructed over 40 years earlier. Also, the most recent study on California populations of steelhead life history were conducted 70 years earlier. The study is based upon the conceptual model and experimental findings of Thorpe et al (including Mangel, one of hte PIs) on Atlantic salmon. Thorpe et al. determined the timing of cues used by salmon to smolt was influenced by growth history. Mangel et al. propose to use the same approach with steelhead. This is an ambitious and fairly complex proposal, complete with preliminary data and experiments as evidence for feasibility. There are multiple questions (hypotheses) addressed separately within laboratory, field and modeling components of the study. The laboratory studies will determine timing of decision windows and thresholds that indicate if parr will migrate or stay. Much of the lab studies were based upon preliminary experiments

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Technical Synthesis Panel Review

conducted by an undergraduate student, and were not available for review, or were well described in the proposal. In this proposal, steelhead raised from fry will be exposed to different rations, and their growth, behavior, ATPase levels and color monitored to examine timing of decision windows and thresholds for migration. For the laboratory component, determining a behavioral index for smolting seems the weakest part, and can be addressed just as well with ATPase monitoring and the seawater readiness test. It wasn't clear how motor activity would be quantified, or shelter use. The field experiments will determine variability in growth potential as a function of prey density and flow conditions. The PIs propose to estimate density using multiple methods (snorkeling, electrofishing, seines), without discussing how they will reconcile or calibrate estimates. I recommend the PIs stick with one method that will work regardless of variable water clarity conditions. The estimation of prey density is to be commended, as it is often lacking in many studies. However, there is no mention of relating steelhead growth to independent variables (steelhead or prey density, flow, etc.).

Additional Comments:

The modeling component will integrate what is learned from the field and laboratory component to project fitness of steelhead populations under different growth regimes and smolt decisions as affected by different hydrologic conditions. Mangel is an expert with this type of analysis. The main problem I had with this analysis, and the field component is there is no mention of survival rates. Survival is a key component of steelhead population dynamics, and is critical to the modeling of fitness. If steelhead decide to leave the stream later, then will mortality be high enough to adversely affect their population numbers? How will mortality be estimated from stream data? How will mortality be differentiated from emigration rates? Also, will mortality rates in the ocean phase of the life cycle be adjusted by the size at which steelhead smolt? It is well known that younger, smaller smolts experience higher ocean mortality than do larger, older smolts, usually due to predation in estuary or near-shore

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environments. How will this be dealt with in the model? What values will be assumed for ocean phase mortality? Other points: The laboratory experiments seemed well designed but overly ambitious. The numbers of replicates, numbers of individuals within tanks due to attrition, behavioral studies, and enzyme sampling together could cause undue stress to the fish, and thereby confound growth measures. The field components were ambitious and vague in sections, but based upon the PIs record, they should make suitable decisions.

The goals of this study are to determine, using laboratory, field and modeling studies; (1) how environmental factors determine steelhead life history transitions; (2) how steelhead life history patterns vary across populations and watersheds, (3) how flow modification affects growth opportunity and life history decisions; and 4) how life history transitions affect steelhead population dynamics. The justification for this project is high, as steelhead populations have continued to decline since dams were constructed over 40 years earlier. Also, the most recent study on California populations of steelhead life history were conducted 70 years earlier. The study is based upon the conceptual model and experimental findings of Thorpe et al (including Mangel, one of the PIs) on Atlantic salmon. Thorpe et al. determined the timing of cues used by salmon to smolt was influenced by growth history. Mangel et al. propose to use the same approach with steelhead. This is an ambitious and fairly complex proposal, complete with preliminary data and experiments as evidence for feasibility. There are multiple questions (hypotheses) addressed separately within laboratory, field and modeling components of the study. The laboratory studies will determine timing of decision windows and thresholds that indicate if parr will migrate or stay. Much of the lab studies were based upon preliminary experiments conducted by an undergraduate student, and were not available for review, or were well described in the proposal. In this proposal, steelhead raised from fry will be exposed to different rations, and their growth, behavior, ATPase levels and color monitored to examine timing of decision windows and thresholds for migration. For the laboratory component,

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Life history variation in steelhead trout and the implications for water management.

The proposed study will provide valuable current information on steelhead life history. The study has strong management implications. There is a strong theoretical basis for looking at life history transition stages. The proposal would investigate rapid growth cues as a mechanism underlying important life history variation in steelhead. Polymorphism in fish life history patterns (resident, early migrant, or late migrant) in watersheds is more widespread than is largely realized. Understanding the life history polymorphism is critical for predicting population responses to watershed management. This study would add valuable theoretical analysis because only one extant model is available in the literature (Atlantic salmon). The proposed experiments in the proposal are well designed to test hypotheses, but lacked some detail. This was a strong proposal, but details of the lab experiments were wanting for some reviewers; also statistics were not well described. Multiple density measurement protocols were questionable. The survival of fish was not adequately

Technical Synthesis Panel Review

considered in the model or in field components. The PIT tagging element of the study was poorly described.

Final Ranking: Above Average

Technical Review #1

proposal title: Life History Variation in Steelhead Trout and the Implications for Water Mangement

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The overall goal of this project is to determine how water management regimes may impact the life history trajectory that juvenile steelhead salmon (<i>Oncorhynchus mykiss</i>) follow.</p> <p>The ideas driving this research are that 1) growth rate of juvenile fish plays a key role in determining the age at which fish smolt and subsequently migrate to sea; and 2) growth rate, in turn, should be impacted by changes in water flow because water flow is likely to influence both temperature regimes fish experience and food available to them at critical times in their life history. 3) Empirical data for ideas 1 and 2 can be used in a predictive life-history model that can help direct water management decisions and thereby help to sustain native populations of salmon in the future. The problem is timely and important. The researchers accurately point out that, in California, nearly every ESU of steelhead is listed as either threatened or endangered under the Endangered Species Act. The environmental conditions that juvenile steelhead salmon require need to be better understood with respect to water management impacts on habitat quality.</p> <p>With respect to clarity, I did not find this proposal written to a professional standard. For example, this is the sentence that introduces "Goals, Objectives and</p>
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Technical Review #1

	<p>Hypotheses”:</p> <p>“The overall goal of our proposed research is to extend the framework developed by Thorpe et al. (1998) for Atlantic salmon to California steelhead, using field and lab studies to derive appropriate empirical data and modeling to modify and refine the theory for Central Valley and central coast populations and apply it to investigate the effects of different flow regimes on steelhead population dynamics.”</p> <p>As this sentence illustrates, questions and hypotheses to be tested were seldom stated plainly. A lack of internal coherence made the proposal difficult to follow. The “Project Purpose” was not well articulated, with key sentences expressing goals or hypotheses to be tested buried six or seven sentences into a paragraph (e.g., page 2, para 3). The “Background” section was redundant and wordy while descriptions of experiments to be performed were brief and did not provide adequate detail to judge whether or not goals could be achieved. For example, the nine hypotheses listed on page 7 are somehow linked to five questions listed on page 8 without a clear mapping or correspondence to the experiments that will be performed. Hypotheses are not carried over into an articulate experimental design for each of the tasks proposed. By contrast, the modeling section is straightforward, drawing clear links to earlier work with Atlantic salmon (in particular Thorpe et al. 1998). Unfortunately, the success of its implementation rests on the integrity of the data collected through experimental and field work which appears to be not well thought out.</p>
Rating	fair

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection

Technical Review #1

of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>While this project seems to lack scientific rigor in its description, the ideas behind this proposal have a great deal of intellectual merit. This justification is beautifully stated on page 14. The researchers point out that their proposal touches on several key CALFED areas including: life cycle models of key species, environmental influences on key species and ecosystems, direct and indirect effects of water diversions on at-risk species and salmonid related projects. Certainly the idea of collecting solid empirical data to fuel sound theoretical understanding of salmonid life history (with implications to water management issues) is fundamentally important, not only for steelhead, but for other salmon species as well.</p> <p>The researchers do an adequate job of pointing out the need for a better understanding mechanisms underpinning the diversity of life history trajectories expressed by steelhead in particular. The conceptual framework is most clearly stated in the modeling component of the proposal, though critical ideas are still glossed over, perhaps to economize on space. For example, having some idea of the distribution of "thresholds" for smolting is critical to the success of this model but how the researchers are going to be able to get his information is not well explained. Nor was it clear how individual life history patterns would be scaled to population level questions. I would have also expected more detail to be applied to how general models could be applied to coastal and central Valley fish. Insufficient preliminary results are provided to insure that the goals can be met or to justify the scope of the work proposed.</p>
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>Just to recap, the overarching goal of this proposal is to determine how water management regimes may impact the life history trajectory that juvenile steelhead salmon (<i>Oncorhynchus mykiss</i>) follow. The focus of the proposed research is thus to determine how different "proximate factors" impact growth and consequent life history pathways (i.e., decision windows for smolting). The link the investigators need to make is between these "proximate factors" and how variability in the watershed impacts growth; in other words, how water management decisions can influence food availability, temperature and habitat usage.</p> <p>Laboratory experiments apparently focus on the timing of the 'decision window' is set for emigration to sea. These 'experiments' are described on pages 8-10 in a somewhat disorganized narrative. While a list of "questions to be addressed" is provided at the beginning of this section, the researchers do not provide an outline of their experimental design or what specific hypotheses are to be tested. For example, with respect to what I assume to be "rearing studies", they state that fish will be collected from two different hatcheries at different locations within California - Scott Creek and Coleman National Fish Hatchery. Considering how little preliminary data they have, wouldn't it make more sense to focus on a single population for initial studies? They state that fish (how many?) will be transferred to aquarium facilities at NMFS Santa Cruz lab but provide no information about the capabilities of these facilities. The suggestion is that these are indoor, temperature controlled facilities with access to flow-through, chilled water that can adequately replicate an</p>
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artificial habitat midway between a coastal and Central Valley habitat, or whatever is implied here. However, they have also budgeted a portable chiller unit (\$5,000) which suggests a more make-shift set-up. This experiment depends on providing strict control over the rearing environment. A detailed description of the facilities needs to be provided to determine whether the experiment is feasible and will produce useful data. With respect to rearing, no formal rearing protocol is provided or cited and many details have not been considered. For example, what will be the rearing density and how does this compare to rearing density in natural streams? How will feeding rate per individual be assessed? Since there will be competition among individuals for food, how will rations be "adjusted" within tanks? Perhaps the investigators were simply in a hurry to reach a deadline, but I am left with the feeling that none of the details of these experiments have been well thought out.

The description of the behavioral studies is too cryptic to judge. It is not clear what hypotheses are being tested and only a hint of an experimental design is provided. For example, why are fish being tested in dyads? How will "motor activity" be defined and quantified? What is meant by "shelter use"? (page 9, line 14; up until this casual mention, shelters have not been described). How will data be scored and analyzed? For example, will data be scored in real time? Will behavioral analysis be implemented by commercially available software? If so, they haven't budgeted for it.

With respect to physiological parameters, statements like "we will monitor physiological condition of the fish in our experiments by measuring swimming capability and metabolic rate in a swim tunnel respirometer once a month, using a representative sample of fish from each population treatment" require a protocol (!) if we are to believe the researchers

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have thought out their experiments in advance. Since surges in thyroid hormone are linked to both metabolic activity and the onset of the parr-smolt transformation, it is not clear why this important index is missing.

With respect to monitoring early maturation (jacking), it is again unclear what specific hypothesis is being tested or how feeding rate will be accessed.

Field experiments: The stated goal is "to examine the variability in growth potential as a function of different environmental conditions and to document the breadth of responses exhibited in natural systems by young steelhead." How food availability varies with water flow in these different regions is filling an important gap in our knowledge, but again, no experimental design is provided. There are no clear hypotheses being tested and it is difficult to judge what useful information can be gained without a logical framework. With respect to methodology, again, the protocols are not well described, but at least here they seem to be appropriately referenced. Estimating fish density is less clear. For example, how will the different methods used (e.g., daytime snorkeling surveys, electro-shocking, PTT monitoring etc) will be calibrated to each other? This question seems critical since it appears that different methods will be used in different water sheds. Since no statistical design is provided, it is difficult to judge how data will be impacted. With respect to electro-shocking and mark recapture methods, there is no mention of potential mortality to juvenile fish.

The investigators "expect to encounter a broad range of environmental conditions across [the three year study period] providing the maximum variability in growth potential experienced by natural steelhead populations in the four watersheds". Although statements such as this one sound far reaching, I would like to see a more stable bridge to how these

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	data and results from empirical testing feed into the modeling component and water management.
Rating	poor

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Despite their claims to the contrary, the investigators have failed to convince me that they have "clearly demonstrated feasibility of each component of the project". There is no hint at a statistical design for any experiment proposed; it is not clear what kind of sample sizes these experiments will require. Little preliminary data is provided, and experimental details have not been adequately described to predict success. Given the budget that is being requested, it is indeed remarkable that the only hard preliminary data cited (i.e., on the effects of size and food rations on Na ⁺ /K ⁺ ATPase levels as indicators of smolting) comes from an apparently unpublished undergraduate thesis co-advised by Mangel and Sogard. This work was conducted over two years ago which either speaks to its quality or a miss on the part of the advisors to get the work out in a timely way.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	No statisitcal design is described for any of the experiments proposed. Treatment-control comparisons are not described. Please see my additional comments under "feasibility".
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Rating	poor
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Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The first step in a large-scale project such as this one is to craft a proposal that provides a well articulated plan for the work that is to be done. It might be that these researchers are submitting this document as a rough draft to get ideas from reviewers, but as it stands, the quality of the proposal currently predicts a poor outcome.
Rating	fair

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Mangel is a Professor of Applied Mathematics and Statistics at University of California, Santa Cruz. He is widely regarded as a leading expert in dynamic state variable models. He literally wrote the book on it (Clark and Mangel, 2000). He also co-authored one of the classic papers in this field with respect to salmonid life-history variation (Thorpe et al., 1998). Given this framework, the modeling aspects of this proposal are its strongest suits. While Mangel is highly capable as a modeler, he is not strong working with animals in the field or as an experimentalist. He seems to do his best work when he is teamed up with
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Technical Review #1

	<p>strong biologists, and cannot be expected to drive the empirical work in this proposal.</p> <p>Sogard is a Supervisory Research Fishery Biologist at NMFS in Santa Cruz. Her current position (since 2001) apparently includes supervising research in salmon ecology, but she has no papers published in this area (at least none are cited in this proposal or listed on her CV). She does, however, have a good publication record dealing with growth questions in marine fishes. The work is budgeted to be carried out by an unnamed postdoc under her direction. Without knowing the background of this individual, it is impossible to assess whether the goals of the empirical studies can be met.</p> <p>Titus is a Senior Environmental Scientist at California Department of Fish and Game, Stream Evaluation Program, Sacramento, California. He lists a total of five peer-reviewed publications. Four of these publications seem pertinent to this proposal, but these contributions are more than ten years old (1988-92). Based on his technical background, he appears to have a background in stream ecology adequate enough to carry out this part of the proposal.</p>
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Task 1-2: Extend Theory of Thorpe et al. The budget seems substantially over budgeted for the work proposed. It is not adequately justified why a full time postdoc and research assistant are needed to do the modeling work when Mangel has budgeted himself so generously (the equivalent of 1 yr associate professor's 11 month appointment at a UC campus) to perform
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Technical Review #1

	<p>the same task.</p> <p>Task 3-5: Laboratory growth / maturation experiment. Insufficient information is given in the experimental design to justify the costs of this project. The need (and estimated cost) for the laboratory chiller unit would indicate that the facilities are not sophisticated enough to allow for flow-through temperature control. More detail is needed.</p> <p>Task 6-8: Field experiments: costs seem reasonable and adequately justified.</p>
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	<p>The overall goal of this project is to determine how water management regimes may impact the life history trajectory that juvenile steelhead salmon (<i>Oncorhynchus mykiss</i>) follow. While there is considerable justification for this type of study, the proposal fails in providing a coherent plan to reach this goal. The strengths of the proposal lie with integrative framework combining field, empirical and modeling; however the strength of the model depends on the quality and completeness of the empirical data that feed it. The experimental design for the empirical work and associated preliminary data are insufficient to justify a budget of 1.3 million dollars.</p>
Rating	fair

Technical Review #2

proposal title: Life History Variation in Steelhead Trout and the Implications for Water Mangement

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The overall objective is clearly described and simple, testable hypotheses are identified for each component of the study. The study addresses important aspects of steelhead life history in relation to environmental conditions such as flow regime that are an active target of management. This study could lay the groundwork for similar flow regime - life history - recruitment modeling in other systems, so could be extremely valuable to fish biologists and water managers.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The study builds on a conceptual model developed for timing of smolting in Atlantic salmon, while recognizing the need to adapt the model to accommodate the unique environmental conditions encountered by steelhead at the southern edge of their range. They have obviously consulted the literature
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Technical Review #2

	in depth, building on laboratory and field studies on similar questions. It appears that they have already conducted considerable pilot work related to this study so full implementation seems warranted.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is well designed and very well thought-out. The study is very ambitious but they have broken it down into complementary components that they seem to have the ability to complete. The study would provide valuable knowledge about steelhead life history and production in relation to flow regime, which would clearly be beneficial to managers. Beyond this local applicability, however, I believe this study would be useful as a conceptual framework for other studies on fish life history, production, and flow regime across many different kinds of systems.
Rating	excellent

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	See previous comment. The approach is documented in thorough detail, with evidence (preliminary results) that they have the ability to conduct this research.
Rating	

Technical Review #2

	very good
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Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Not applicable.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The results will be published in quality journals (as the authors' Cvs attest to), and these papers will be made readily available. The value of these publications is likely to be high, extending beyond just topics pertaining to steelhead. Will the data be expressed in a form directly useful to local managers, however? There is no mention of disseminating the data either. The field data in particular seem like they would be very useful to managers and other researchers - what are the plans for archiving and sharing data?
Rating	good

Additional Comments

Comments

Technical Review #2

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors have outstanding track records in research similar to the proposed study. In fact, the primary author wrote the book on confronting models with data ('The Ecological Detective') - which is exactly the type of study proposed here. They seem to have adequate infrastructure to conduct the work and have set up cooperative agreements with hatcheries to spawn the fishes needed for the laboratory work.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is high, but it is an ambitious project and will likely be a good return on the expense.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	I may be biased towards really liking this proposal, because I have worked on projects involving determining fish population and life history responses to variation in flow regime - and I appreciate the complexity of such questions. Therefore I am impressed with the way they propose to integrate fieldwork, lab work, and modeling into a cohesive conceptual model, and I see the usefulness of this study extending far beyond questions about California
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Technical Review #2

	populations of steelhead. I do not know any of the authors personally but I am familiar with their work and their Cvs attest to their ability to conduct sound and relevant research.
Rating	excellent

Technical Review #3

proposal title: Life History Variation in Steelhead Trout and the Implications for Water Mangement

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>This proposal states that the goals of this Central Valley-Coastal steelhead trout study are fourfold. The list of goals is to determine: 1) how environmental factors determine life history transitions, 2) how these patterns vary across different populations and watersheds, 3) how flow modification impacts growth opportunity and life history decisions, and 4) how these life history transitions affect population dynamics. The proposal authors, Mangel et al., hypothesize that through the use of "three-pronged approach" of laboratory, field, and modeling studies they will adequately address theses goals. The effects of streamflow and environmental conditions on subyearling steelhead will be studied using the comprehensive study plan and results of all three approaches will be used to develop predictive models of life history consequences under varying environmental conditions and water policies. The objectives of this project remain internally consistent throughout the proposal. In the "goals" section of the proposal nine different hypotheses are listed under the respective experimental approach that will test them. The lab portion will address 5 different hypotheses while the field studies will be guided by the remaining four. These hypotheses form the questions and guidelines that are presented in each section throughout the remainder of the proposal allowing the reader to follow along without having to</p>
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Technical Review #3

	<p>turn back to the "goals" section to serve as a reference. One of the reasons this proposal is so well organized is that it is applying a successful study design previously used by Thorpe et al. 1998 who was studying Atlantic salmon smolting/emigration. Mangel et al. present several examples from the Atlantic salmon study and clearly explain how they will modify the approach to study the same general question with steelhead trout in California. No ambiguities or hidden agenda is apparent; everything appears to be clearly presented. The underlying rationale for doing this study is the continuing decline of steelhead populations throughout the state, with all but one ESU being ESA listed. Mangel et al. state that even though major dam construction ceased 40+ years ago, there is a need to determine why populations are continuing to decline at such an alarming rate. This type of study looking at Pacific salmonid life history strategies is long overdue. There has been a recent increase in interest in regards to steelhead in the Central Valley, but the last time a comparative study like this was done on coastal O. mykiss was over 70 years ago (Shapovalov and Taft 1954). This study proposes to simultaneously investigate steelhead populations in two coastal watersheds and in two Central Valley tributaries of the Sacramento River. In addition to the field components, the laboratory experiments should also provide valuable information that will be applicable to wild fish populations since the experimental fish will come from 'conservation-type' hatcheries that use wild captured broodstock from the same watershed where the field studies will occur.</p>
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Technical Review #3

Comments	<p>This study is testing a justifiable question that should provide a better understanding of what cues steelhead trout to remain in freshwater or emigrate to the ocean. The existing knowledge on this topic is limited. Inferences have been drawn on the work Thorp et al. did with Atlantic salmon and based on hatchery programs that have experimented with releasing (marked) steelhead volitionally. Drawing conclusions from artificially produced hatchery fish on how wild fish populations function has its obvious limitations, as does using an Atlantic salmon species as a surrogate for Pacific coast steelhead based on the fact that both species share the iteroparous life history. A conceptual model explaining the underlying basis for the proposed work is not included in the proposal. Because of this, I found the "project purpose" and "background section" to be the weaker parts of the proposal. There were several times I was ready to turn the page and see a model tying the entire 3-year project together, but instead I kept turning pages. Since this project is composed of a three-prong approach, a model would have helped facilitate the reader's comprehension of how and when each of the components would be carried out and how they would compliment one another in building the final model. Even a diagram/model describing the laboratory component of the project would have benefited in the visualization of the different experiments. Justification is provided in this proposal to fund this study as a full-scale three year implementation project. This is due in part to the lack of uncertainties that exist with the study methodology. Mangel et al. draw repeatedly on the work of Thorpe et al. to justify the underlying rationale for this project, and it is clear that the</p>
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	research team has crafted a well-designed study plan to answer their questions in regards to steelhead life history decisions. Each phase of the project contributes to one of the five topics presented in "Attachment 1" of the CALFED call for proposals. It appears all components of this multi-year study are necessary to fulfill the objective of building the final model. Mangel et al. made it clear that their intensive study design will need to be funded for all 3 years not only to draw comparisons between different environmental conditions, but also to ensure completion of all project tasks.
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Since Mangel et al. present a Three-pronged approach, I will address each one of the project components separately to determine if the methodology is appropriate for the questions being asked. All three study components will produce novel information if the experiments proceed as planned. Approach feasibility will be addressed in section 4. The laboratory experiments will allow the researchers to manipulate fish from each of the study areas (Central Valley and Coastal stocks) in a controlled setting. The main purpose of these studies is to examine the timing of decision windows and thresholds that indicate if a parr will stay or migrate. It is a big plus that the fish are being collected from hatcheries practicing conservation protocols to ensure that the fish used in these experiments are as genetically similar to the wild stocks as possible. There are several questions
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(n=5) being tested in the laboratory experiments, and many should provide results likely to add to the base of steelhead migratory behavior. The questions or concerns that I have with the laboratory experiments are:

- In regards to the 2-month fast growth study, it is unclear (i.e. unstated) why the researchers "expect the decision window to be limited to one of the [two-month] periods". This very well might be a result based on Thorp et al. results, but it is not explained and it left this reviewer wanting more information.
- Not enough detail presented on how growth will be monitored. Will fish be marked for identification? Will all fish from each treatment be averaged for a mean tank value? The methodology used in collecting the growth metrics are important details to leave out.
- Behavioral Study: This is the weak link in the laboratory component. What will the behavioral component add to the model that the seawater readiness test won't already provide? Not only do I feel this portion of the experiment is more theoretical than applied in nature, but I find it difficult to believe that the circular tanks will "match the natural physical conditions of the two [river] systems". If it is necessary to incorporate behavior aspects into this study, I say scratch the tanks and set up mesocosms in the respective watershed to better evaluate activity levels in the natural stream settings.
- The 'increased silvering' factor seems too subjective to include as a possible index; once again, won't the ATPase levels give a quantitative measure to evaluate readiness for emigration?
- Will fish growth continue to be monitored during fish treadmill respirometer trials? If so, won't these tests and increased handling have a negative effect on growth?
- I do not believe it is necessary to compare seawater readiness with Na+K+ATPase levels to verify the ability of the enzyme assay to predict smoltification. Why reinvent the wheel when "ATPase studies have already answered this question? See Richards et al. 2003 (JEB) for a recent paper looking at this in *O. mykiss*, or for a review see McCormick 1995. The field component of this

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study appears to be solid. I only have a couple of comments regarding the design. • USGS water flow gauges on the American River and Soquel Creek will greatly facilitate data collection. Will sites be located in close proximity to the actual gauges, or will rating tables be constructed to account for channel shape and morphology as well as input and diversions downstream of the gauges? No mention is made as to how flow data will be collected at Scott Creek and the Mokelumne River when personnel are not on-site with a hand held flow meter. Another practical concern for sites without a USGS gauge will be collecting flow data during periods of high flow. Coastal systems are very flashy. Winter freshets are capable of making Rivers rise >10' in very short time spans. If winter flow data is only collected when it is safe to do so, then a large portion of the hydrograph will not be represented in the study design for the two systems lacking gauges. These winter flow events may play an important role in defining the life history decisions being tested by this study. • Not clear why the calculation of fish density estimates will be carried out using two different methodologies? If summer stream conditions will prohibit the use of snorkel surveys, then why not use E-fishing depletion estimates from the start? If the two different techniques are used, then the methods must be calibrated to in the different watersheds to account for biases. If there is reason to believe that visibility will become a seasonal issue then plan ahead and sample using seines in the beginning. The modeling component of this project demonstrates that Mangel et al. have carefully predetermined what data is needed for building a successful model. One logistical concern I have regarding the model is that Mangel et al. state that juvenile salmonids have only three choices. Those choices are emigrate, remain in freshwater, or mature precociously. A reference made in the "Modeling component" section regarding this states that, "If the projection is greater than the threshold, then the fish commits to a developmental

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	<p>pathway leading to smolting; otherwise it is resident in freshwater another year". An important alternative pathway that is not mentioned is parr-revertancy, in which some <i>Oncorhynchus</i> spp. have been documented to revert back to parr after successful smoltification. Maybe this topic wasn't brought up because Thorpe et al. didn't mention it, so Mangel et al. also decided not to address it. My guess is that this wasn't the case, because researchers have not been able to get Atlantic salmon smolts to reverse the parr-smolt transition while Pacific salmon have been observed making the change (see Folmer et al. 1972). If light of this possible alternative pathway, as unlikely as it may be, the model should accommodate the fact that it can happen. Mangel et al make mention of how the coastal watersheds have sandbars close off the river from the ocean during the dry summer months. In times of drought, there is the possibility that these rivers wouldn't breach for extended periods (like in Southern CA). When these major shifts in the environment occur, any smolts migrating downriver would be forced to adapt or die.</p>
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>The feasibility of this project is quite possibility its strongest selling point. All project components have been demonstrated by pilot studies in the lab, field and prior modeling studies. The lab experiments that were conducted by Megan Atcheson helped define the protocol that will guide the laboratory studies presented in this proposal. Tagging studies in Soquel Creek indicated that collecting and recapturing adequate numbers of fish as well as determining that differences</p>
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	in seasonal growth rates did occur helped establish the field protocol and modify the existing life cycle diagram established for Atlantic salmon. The scale of the project spans from the coast to the central valley, but the project remains within grasp of Mangel et al. because Rob Titus will supervise the field crews on the central valley rivers. This project has a relatively high likelihood of success in determining if an emigration threshold does exist for steelhead trout by drawing on comparisons between three very different settings: central valley big rivers, smaller/cooler coastal creeks and a laboratory setting.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	There is a likely chance that water management implications may result from this project. If flows play a major role in the delivery of prey items to steelhead, then measures of growth, body condition and the timing of anadromous fish 'decision windows' may provide useful information for water management operations. If this project is able to assist fishery
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	<p>managers make more informed predictions regarding the timing of smoltification and steelhead decisions to emigrate, then water diversions and pumping facilities could adjust their schedules accordingly. Possibly the most interpretive outcome of this study will be determining the critical window of opportunity and associated performance measures that cause Pacific salmon spp. to migrate downstream or remain in freshwater another year. It is likely that both the field and lab components will compliment the other in determining what the factors are that regulate these complex choices made by these fish spp. If so, this model should be able to be easily adapted to other Pacific salmon species. Steelhead have the most variable life history of all the <i>Oncorhynchus</i> spp., so application to other salmonids should prove easier than building the original model.</p>
Rating	good

Additional Comments

Comments	<p>A map marked with study sites would help orient the reviewer with the study system and experimental design. It would have been nice if the authors would have addressed the fact that parr-revertancy does exist and could potentially complicate the Thorpe et al. approach. Because it wasn't mentioned almost makes me think they are aware of the problems that it could present. I feel that portions of the laboratory component should be eliminated, and if that leaves a lab tech with some free time then they could help sort and identify the invertebrate samples since that will be one of the rate limiting steps in finishing up with all analyses in a timely fashion.</p>
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	<p>The Primary Staff is composed of a solid team of scientists that have extensive large-project management experience. The proposal is well-written in regards to the areas of specialization of the different team members. Both M. Mangel and S. Sogard have extensive experience working with anadromous salmonids. Mangel has been a professor at UCSC for the past eight years, and contributes on numerous other fishery panel memberships (i.e. Assessing Extinction Risk for West Coast Salmon). Sogard has been with the NMFS as a Supervisory Research Fishery Biologist for four years working predominantly on the life history strategies of fishes. Mangel and Sogard have an excellent track record managing large projects and have already demonstrated that they work well together evidenced by their co supervision of Megan Atcheson. R. Titus is the third member of the primary staff for this project. Titus has been with the CDFG for nine years as a Senior Environmental Specialist working on salmon and steelhead projects. Mangel, Sogard and Titus all have an extensive list of publications in leading professional journals. The infrastructure within the NMFS Santa Cruz Lab and the Central Valley CDFG office will facilitate this project in many ways. Project management by the co-project leaders located in Santa Cruz will lend strong support for the success of the project. Joe Merz, with East Bay Municipal District, has offered his invertebrate identification expertise in training and use of his lab to analyze the samples.</p>
Rating	very good

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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget appears to adequately represent the work outlined in the proposal. The only equipment requested to carry out the study which is not already owned by is the chiller to control water temperatures. There are two parts of the budget that I disagree with. The first is the request for almost \$10000 to purchase invertebrate sampling gear. This figure is a gross overestimation. The other part of the budget I think that could be cut out entirely is the behavioral and the fish photography study to monitor increased silvering experiments. These reductions could possibly bring the total cost of the project just under \$1,000,000.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	<p>Below is a bulleted list of pluses (+) and minuses (-) I used in evaluating my overall review of this proposal.</p> <p>+ Primary staff is highly experienced and capable of managing large-scale projects. + Addresses a question that will give fishery managers first measure of smolt decision cues + Project team has experience working with salmonid migratory behavior + Project team has experience working together on previous projects + Justification of methodology exists for each component of the this project + May provide a better understanding of the relative importance of flow impact on parr decisions + Could lead to better water</p>
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	management/operation decisions - Modification of the experimental design regarding parr-revertancy should be considered - Authors don't link their research to doubling populations of naturally produced salmon - Clarification is needed on how growth will be measured in lab experiment (tank means?) - Fish collection methods for fish density calculations should not change during the experiment - Not necessary to measure smolt seawater readiness to validate gill ATPase, see literature
Rating	good